I. Wagner, H. Tellioglu, E. Balka, C. Simone, and L. Ciolfi (eds.).
ECSCW'09: Proceedings of the 11th European Conference on Computer Supported Cooperative Work,
7-11 September 2009, Vienna, Austria

Divided by a common acronym: *On the fragmentation of CSCW*

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Abstract. CSCW is in an advanced state of fragmentation. The acronym now, by and large, denotes widely diverging research programs that, apart from a shared name, have little or nothing in common. This situation obviously calls for clarification. Recounting the prehistory and formation of CSCW, the paper shows that CSCW, as a distinct research program devoted to the development of new technologies on the basis of understanding actual cooperative work practices, arose in response to the crises in which 'Computer Mediated Communication' (CMC) and 'Office Automation' (OA) had landed by the late 1980s. The paper finally discusses the reasons why CMC, although superseded as a research paradigm by the practice-oriented program of CSCW, has gained a new lease on life in CSCW and thus why CSCW has become fragmented.

Development of technology results in technical knowledge, methods, principles, etc. That is, it is essentially a conceptual effort. CSCW research therefore has to be cumulative. For CSCW research to be cumulative does not mean a linear process, of course, but a process in which the different contributions — empirical studies, conceptualizations, experimental designs, architectures — build upon, corroborate, exemplify, complement, generalize, question, discuss, subvert, or overthrow other contributions. However, in a 'degenerating' research program, to use the term coined by the philosopher of science Imre Lakatos (1970), this process is effectively blocked. The required continuity of the program, the ongoing development of concepts and frameworks, is replaced by restless reformulation of the research problem, slapdash changes of scope, unaccountable redefinitions of key concepts, etc. Under such circumstances the progressive development of the conceptual foundation of technology is not possible.

Now, CSCW was never a well-defined research area. In 1988 Liam Bannon *et al*. observed that 'for the moment the name CSCW simply serves as a useful forum for a variety of researchers with different backgrounds and techniques to discuss their work' (Bannon, *et al*., 1988). But as indicated by the temporal modifier 'for the moment', Bannon *et al*. obviously expected this state of affairs to pass. Others, however, saw in this condition a virtue of the field: 'Perhaps paradoxically, one of the most refreshing things about CSCW may be the fact that its meanings [are] debated. For as long as this is the case, researchers will reflect upon the nature of their work, what its aims and outcomes are or might be' (Bowers and Benford, 1991, p. 1). Neither of these expectations have been fulfilled. In fact, CSCW has become fragmented. The upshot is that CSCW as a research field is unable to proceed in anything like a cumulative fashion and thus, generally, unable to contribute systematically and constructively to the development of new technologies.

One example will suffice to indicate the level of fragmentation. Take the review article in the *HCI Handbook*, entitled 'Groupware and computer-supported cooperative work' (Olson and Olson, 2003). Written by two eminent CSCW veterans, Gary Olson and Judith Olson, the article gives an overview of a range of types of application such as email, conferencing, instant messaging, group calendars, shared repositories and work spaces, media spaces, and collaborative virtual environments. Although the authors assert to be using the terms groupware and CSCW 'quite broadly' (p. 584), they nonetheless conceive of the field quite narrowly as a field focusing on technologies that, in different ways, support 'collaboration' or 'group work' 'at a distance' (*ibid*.).

This account is puzzling. First of all, it assimilates CSCW with the research area of Computer-Mediated Communication (CMC) that preceded it by many years. But what makes it quite remarkable is that it leaves out the significant body of CSCW research that involves investigations of cooperative work practices in professional settings (such as air traffic control, maintenance work, urban rapid transit control, software engineering, manufacturing, health care). This entire body of research is not reflected in the review at all. In fact, this review of CSCW completely ignores the substantial contribution of ethnographic or workplace studies to CSCW. Terms such as 'ethnography', 'workplace study', or 'field work' do not even feature in the article, nor are there any references to particular ethnographic studies or to the large CSCW literature about the role of workplace studies.

This is remarkable because ethnographically informed CSCW research has had deep and lasting impact on the scope and direction of major sectors of CSCW due to the way in which in-depth studies and sociological analysis of actual cooperative work practices have been made to bear on conceptual issues of technological research and development. Furthermore, exactly by virtue of this paradigmatic kind of socio-technical inquiry, CSCW has exerted significant influence on related scholarly fields such as human-computer interaction (HCI), participatory design (PD), and science and technology studies (STS).

As a researcher who, for decades, has been involved in ethnographically informed CSCW research, I was, at first, appalled at a review of CSCW so conspicuously partial. But then again, the picture painted by Olson and Olson is also, in a sense, a genuine reflection of the actual state of affairs in CSCW, and I could easily imagine another review article, from another quarter, that was equally partial. In that light, moral outrage is unwarranted. What we have, it seems, is rather a situation characterized by 'incommensurate paradigms' (Kuhn, 1962): different research programs that have little or nothing in common, apart from a shared acronym, addressing different problems, conceived of in different conceptual frameworks, employing largely disparate methods and techniques of research. But what we have is not a classical paradigm crisis. The mutual indifference of the different programs in CSCW is rather an indication of fragmentation.

This situation calls for clarification of the distinct features of the different re search programs. The present paper is meant as a contribution to this process. There are other fault lines and other sources of fragmentation in CSCW, but for obvious reasons the paper will focus on showing the fundamental differences between the research program represented in the review article by Olson and Olson and the practice-oriented program. An initial look at the prehistory and formation of CSCW will show that the practice-oriented program of CSCW emerged in response to internal problems in CMC as it was then conceived and in other contemporary research areas and will thereby help to clarify what was and is specific in CSCW. The purpose of this is not to write a history of CMC and CSCW but to demarcate the intellectual fault lines.

1. The prehistory of CSCW

The beginning of CSCW is so humble that there hardly is any record of it: small practical steps to deal with mundane practical problems.

We are back in the prehistory of CSCW, in the early days of electronic computers, when the notion of computer-mediated communication was gradually gestated. More precisely, the notion of CMC begins with the notion of 'time-sharing' operating systems that matured around 1960. Computer systems were at that time excessively expensive and it was mandatory that systems were operating close to full capacity. Consequently, the few computers that were around were running in a batch-processing mode, one job after another on a 'first-in, first-served' basis, or, as it was aptly expressed by J. C. R. Licklider, who played a leading role in the early development of interactive computing, the 'conventional computer-center mode of operation' was 'patterned after the neighborhood dry cleaner ("in by ten, out by five")' (Licklider and Clark, 1962, p. 114). This economic regime effectively precluded computer applications such as military command and control, war gaming, air traffic control, computer-aided design, etc. that were of obvious and critical interest to important stakeholders such as the US military and other branches of government. The same 'in by ten, out by five' regime also made programming, especially debugging, a deadening affair. This gave ordinary computer technicians a strong motive for devising alternative modes of operation. So, around 1960 the idea of letting a central computer system service several users 'simultaneously' was hatched. In the words of John McCarthy, one of the fathers of the idea, the solution was an operating system that would give 'each user continuous access to the machine' and permit each user 'to behave as though he were in sole control of a computer' (McCarthy, 1983). The first running operating system of this kind seems to have been the Compatible Time-Sharing System or CTSS which was launched in 1961. The various users were connected to the 'host' computer via terminals and each would have access to the computing power of the 'host' as if he or she was the only user.

Now, the users of the first of these systems were typically engaged in cooperative work. Some were engaged in developing operating systems or other large-scale software projects and were, as a vital aspect of this, engaged in various forms of discourse with colleagues within the same project teams and research institutions, that is, with colleagues already connected to the central computer system. Likewise, software technicians would need to coordinate with system operators about possibly lost files to be retrieved, about eagerly-awaited print jobs in the queue, etc. The time-sharing operating system they were building or using provided a potential solution to this need, and the idea of using the system to transfer text messages from one worker to another did not require excessive technical imagination. As one of the designers of one of the first email systems recalls:

> '[CTSS] allowed multiple users to log into the [IBM] 7094 from remote dial-in terminals[] and to store files online on disk. This new ability encouraged users to share information in new ways. When CTSS users wanted to pass messages to each other, they sometimes created files with names like `TO TOM` and put them in "common file" directories, e.g. `M1416 CMFL03`. The recipient could log into CTSS later, from any terminal, and look for the file, and print it out if it was there.' (Van Vleck, 2001)

A proper mail program, 'a general facility that let any user send text messages to any other, with any content' was written for CTSS by Tom Van Vleck and Noel Morris in the summer of 1965 (*ibid*.). It allowed one programmer to send a message to individual programmers, provided one knew the project they worked on, or to everybody on the same project. The message was not strictly speaking 'sent'; it was appended to a file called `MAIL BOX` in the recipient's home directory. The same year Van Vleck and Morris also devised a program (`.SAVED`) 'that allowed users to send lines of text to other logged-in users', that is, a primitive form of 'instant messaging' (*ibid*.).

The scope of the exchange of messages with these and similar programs was limited by the boundary of the hierarchy comprising the local central computer system and the terminals connected to it. Messages could not travel beyond the event horizon of this black hole.

This world of isolated systems dissolved with the development of network computing. Again the motivation driving the development was not to develop facilities for human interaction, not to mention cooperative work, but to utilize scarce resources in a more economical way. As pointed out by Ian Hardy, in his very informative history of the origins of network email,

> 'ARPANET planners never considered email a viable network application. [They] focused on building a network for sharing the kinds of technical resources they believed computer researchers on interactive systems would find most useful for their work: programming libraries, research data, remote procedure calls, and unique software packages available only on specific systems.' (Hardy, 1996, p. 6).

For Licklider, who also initially headed the development of ARPANET, the motivation for the network was to reduce 'the cost of the gigantic memories and the sophisticated programs'. When connected to a network, the cost of such shared resources could be 'divided by the number of users' (Licklider, 1960). That is, the primary motive was again economic.

Anyway, after pioneering work on the underlying packet-switching architecture and protocols, the experimental ARPANET was launched in 1969, connecting measly four nodes. In the summer of 1971, when the network had expanded to fifteen nodes, a programmer named Ray Tomlinson devised a program for sending email over the network. He recalls that, while he was making improvements to a single-host email program (SNDMSG) for a new time-sharing operating system, 'the idea occurred to [him]' to combine SNDMSG with en experimental file-transfer protocol (CPYNET) to enable it to send a message across the network, from one host to another, and append it to the recipient's MAILBOX file. An instant success within the tiny world of ARPANET programmers, this very first network email program triggered a chain reaction of innovation that within less than a couple of years resulted in the email designs we use today: a list of available messages indexed by subject and date, a uniform interface to the handling of sent and received mail, forwarding, reply, etc. — all as a result of programmers' improving on a tool they used themselves. Within five years or so, the volume of email messages had become one of the heaviest traffic component on the growing network (Hardy, 1996, p. 21), and in 1977 an official ARPANET standard for electronic mail was adopted (Crocker*, et al.*, 1977).

What is remarkable in this story, and what also surprised those involved when they began to reflect on the experience, was 'the unplanned, unanticipated and unsupported nature of its birth and early growth. It just happened. and its early history has seemed more like the discovery of a natural phenomenon than the deliberate development of new technology' (Myer and Dodds, 1976, p. 145). And at a meeting in January 1979, convened to discuss the 'the state of computer mail in the ARPA community and to reach some conclusions to guide the further development of computer mail systems', it was 'noted' as a fact 'that most of the mail systems were not formal projects (in the sense of explicitly sponsored research), but things that "just happened"' (Postel, 1982, p. 2). The history of network email after that is well known. The technology migrated beyond the small community of technicians engaged in building computer networks to computer research in general and from there to the world of science and eventually to the world at large.

That is, as in the case of local email on time-sharing operating systems, network email came as an afterthought, devised by computer technicians for their own use, as a means for coordinating their cooperative effort of building, operating, and maintaining a large-scale construction, in this case the incipient Internet. This pattern would repeat itself, again and again. Email and many other CMC technologies that came later were typically thrown together like the scaffolding at a construction site only to become a main feature, relegating the resulting building itself, which had been the original and official objective, to something close to a support structure (cf. also Gillies and Cailliau, 2000).

1.1. The rise and fall of CMC

The experience that human interaction could be facilitated by computers, as demonstrated by email and other protocols, immediately caught the attention and imagination of technologists, who then enthusiastically began developing a generalization of the message exchange idea underlying email, which was soon dubbed 'computer conferencing' (for an overview of this work, cf. Kerr and Hiltz, 1982). In its simplest realizations 'computer conferencing', in contrast to email, was not restricted to point-to-point message exchanges but supported public exchanges within the forum of the online 'conference', regulated in accordance with some established structure. 'Conferencing' was in fact often advocated as a remedy for the 'information overload' which was seen as an inexorable consequence of point-to-point message exchange (Palme, 1984; Hiltz and Turoff, 1985). The more ambitious experiments, such as EMISARI and EIES (e.g., Turoff, 1972, 1973) and FORUM (Vallee, 1976), explored the rather grand design vision of group communication structured according to some presumptively rational model. Sometimes the experiments allowed for long-term use and thus evolution of 'user behavior' (e.g., Hiltz and Turoff, 1981).

While not a development activity undertaken by technicians for their own benefit, this line of research was still characterized by relatively close coupling of experimental design and evaluation work. For instance, between 1973 and 1975, FORUM was tested in 28 conferences and improvements 'were rapidly incorporated' (Vallee, 1976; Panko, 1977).

Although the experiments with 'computer conferencing' at the time were seen as very promising and reported as very successful, this particular research program ran out of steam. This has to do with underlying conceptual limitations. 'Computer conferencing' research shared with the standard message exchange paradigm the presumption that human communication generally is or can be treated as a distinct activity. True, workers do interrupt their primary work to have conversations and exchange notes, letters, memos about their work (and about other matters). They also, occasionally, put their work aside to go to meetings. For some workers, e.g., managers, the major part of their work day may be spent in conversations and meetings. But apart from managerial work and in the greater scheme of things, conversations and meetings are exceptions, interruptions, 'a necessary evil' perhaps, or simply considered 'a waste of time'. And even when workers engage in conversations and meetings, such discourses are generally related to the state of affairs in their work, to the flow of work, the schedule, the production facilities, the archives, and in their deliberations workers will discuss schedules, plans, schemes, and so on; they will collate, arrange, distribute, present, hand out, walk up to, gather around, point to, gesture at, inspect, amend, etc. all sorts of artifacts.

By the mid-1980s this insight began to mature and be voiced (cf., e.g., Bannon, 1986, p. 443). The CMC research program had landed in a crisis.

The critique of the underpinnings of CMC was expressed clearly and succinctly by Irene Greif in her 'Overview' of CSCW in her influential *CSCW: A Book of Readings* (1988). Having noted the rapid development of CMC from electronic mail to computer conferencing she then observes:

> 'Computer conferencing has since been expanded to support a wide range of "many-to-many communication" patterns. However, when computer conferencing is applied to some task, the model breaks down. The unstructured body of messages is suitable for the free-flowing text of natural language, but does not let us set the computer to work on our problems. Designers who draw pictures, software developers who jointly write code, financial analysts who collaborate on a budget — they all need coordination capabilities as an integral part of their work tools. That means coordination support within the CAD engineer's graphics package, within the programmer's source-code editor, within the budget writer's spreadsheet program. It means support for managing versions of objects, be they pictures, programs, or spreadsheets. It means ways to distribute parts of the object for work by contributing group members, ways to track the status of those distributed parts, ways to pull completed objects back together again. The limit of electronic mail and computer conferencing is that they have such features for managing messages only. CSCW widens the technology's scope of application to all the objects we deal with.' (Greif, 1988, pp. 7 f.)

Greif's judgment that 'the model breaks down' was mirrored in the European CMC research community. This community had emerged in the wake of the European efforts to develop computer networking (cf. Gillies and Cailliau, 2000). As TCP/IP slowly became available in operating systems and developers began to be able to take it for granted, and as the 'message handling' standards stabilized in the first half of the 1980s (X.25, X.400, STML), European CMC researchers such as Rolf Speth, Uta Pankoke-Babatz, Wolfgang Prinz, Steve Benford, and others, organized in the AMIGO project, embarked on what was seen as the logical next step, namely, developing the standards required for putting it all together: email as well as directories, calendars, schedules, and so on.

However, the European CMC researchers soon realized that the 'message-handling' model underlying CMC was too limited (Pankoke-Babatz, 1989). In work practices, communication is normally not a separate activity; it is typically an integrated aspect of doing the work. In fact, exchanging messages usually presumes that work is interrupted. It was therefore considered necessary to be able to incorporate communication functionality in the various domain-specific applications.

On the other hand, the European CMC researchers rejected the 'computer conferencing' paradigm as a way to provide structure to the exchange of messaging. Guided by 'a strong commitment to the actual situation in working life' (Pankoke-Babatz, 1989, p. 20), they repudiated the idea underlying the 'computer conferencing' paradigm of providing 'a new model' of communication. Instead, they aimed at providing a model that 'might be used in the design and implementation' of local and temporary 'patterns' of interaction. That is, instead of deciding on a particular preconceived conception of CMC functionalities and applications, they 'chose [...] to look at activities and the regulations required by a group of people to co-operatively execute a particular activity. The model we want to develop should therefore allow specification of such regulations' (*ibid.*). That is, the aim was to build what one could call an abstract model or a notation that would make it possible 'to model the activities, businesses, tasks, actions or workflow[s], which are performed by a group of co-operating people', so as to, in turn, 'facilitate the required co-ordination and possibly to automate co-ordination, thus reducing the co-ordination effort required of the participants in an activity' (p. 23).

The European CMC researchers knew very well that the development of such computational models and architectures would have to be grounded in 'fundamental understanding of Group Communication processes' (p. 14), which in turn, because of the complexity and variability of working practices, would need contributions from 'sociology, anthropology, economics and political science' (p. 21). Their 'strong commitment to the actual situation in working life' was amply demonstrated in the pre-dominance of the practice-oriented program in the European CSCW research community that began to coalesce as these research activities ended in 1988. It is significant that Greif had reached strikingly similar conclusions: 'Methodologies for testing individual user interfaces don't apply as well to group support systems. As a result, CSCW is looking more to anthropology to find methodologies for studying groups at work in their natural settings' (p.10).

In short, it was becoming clear that the CMC program was deeply flawed in its underlying 'message handling' outlook, in its focus on communication in the abstract, divorced from the work practices of which it normally is an integral part, but also severely limited in the way CMC conceived of the role of empirical studies in technological development. It was becoming clear, at least to some, that in-depth studies of cooperative work practices in 'natural settings' was a prerequisite.

1.2. The rise and fall of Office Automation

At the same time as it was becoming clear to many CMC researchers, especially in Europe, that the 'message handling' paradigm was at odds with typical everyday cooperative work practices and that the paradigm thus had to be overcome, researchers in the 'office automation' movement were arriving at similar conclusions, although their point of departure was of course entirely different.

The 'office automation' movement had begun in high spirits in the 1970s, stimulated by different but intersecting technical developments. As with CMC, the baseline was the advent of computer networks. But the approach was radically different. Instead of conceiving of computer networks as a 'medium', that is, as a facility that regulates human interaction in negligible ways, the OA program deliberately aimed at regulating interaction in significant ways. The seminal idea was that various new techniques for constructing executable models that had been invented made it worthwhile to explore whether and to which extent such representations might be exploited as a means of modeling and regulating 'office procedures' and other kinds of workflows: on one hand, the algebraic techniques for building computational models of distributed systems developed by Petri and others since the early 1960s (cf., e.g., Zisman, 1977; Ellis, 1979) and, on the other hand, the equally sophisticated techniques for constructing complex adaptive models developed under the Artificial Intelligence label (cf., e.g., Hewitt, 1977; Fikes and Henderson, 1980; Barber and Hewitt, 1982). These hopes were soon defeated, however. Experimental applications such as DOMINO turned out to be felt like 'straitjackets' in actual use (Kreifelts, 1984; Kreifelts, *et al*., 1991). Comparable lessons were learned from the CHAOS experiment (De Cindio, *et al*., 1988; Bignoli and Simone, 1989). That is, 'office work' was not at all as easily captured and modelled as had been presumed. Handling contingencies and dealing with inconsistencies turned out to be an essential aspect of cooperative work practices. The 'office automation' program had landed in a crisis of its own.

At this point a new approach to technological research was devised: a few sociologists became involved in the effort to understand the status of 'office procedures' and cooperative work in general, on one hand Lucy Suchman and Eleanor Wynn (Wynn, 1979; Suchman, 1982, 1983; Suchman and Wynn, 1984) and on the other Eli Gerson and Susan Leigh Star (Gerson and Star, 1986).

That this coupling of sociological and technological research would first occur in the 'office automation' movement was hardly accidental. Email and most other CMC technologies were devised by computer technicians *for their own use*. That is, they were developed in a bottom-up and incremental fashion to solve local problems in practices that were well-known to the designers; and as they were found to be of general utility they were then — *post festum* — subjected to standardization and design. Their development did not require workplace studies of any kind. On the other hand, computer-conferencing systems were developed in a proactive manner; they were strictly speaking *designed*. But their design was based on normative models of what was claimed to be rational decision making. not on what was taken to be a well-grounded understanding of an actual practice. By contrast, however unrealistic the experimental designs of the 'office automation' movement turned out to be, nobody were under the illusion that one workflow model would fit all, and each workflow model was presumed to be empirically valid. That is, building technical systems that regulate actions and interactions in the strong sense envisioned by the 'office automation' movement was unproblematically thought to require some kind of analysis and modelling of existing procedures. When the models ultimately turned out not to work as anticipated, the natural next step was to look more carefully at the reality of 'office work'.

This is anyway what happened. And it was also realized, eventually, that the problem was not just with this or that particular model or modelling technique. It was realized that the problem was conceptual. Those early studies of 'office work' indicated that received concepts of cooperative work as mere 'execution' of preconceived 'procedures' were inherently problematic. This point was driven home, emphatically, both by Gerson and Star and by Suchman in her contemporaneous critique of the concept of 'plans' in cognitive science (Suchman, 1987).

This insight was a fatal blow to the conceptual basis of the 'office automation' movement.

2. Enter CSCW

The work of Suchman, Wynn, Gerson and Star had significance beyond these, as it were, *immediate* implications. It also showed, *by way of example*, that not only were in-depth studies of actual working practices possible and fruitful; they also demonstrated that such studies could have strong impact on conceptual issues in technological research.

This, in my view, was the defining moment of CSCW. The early contributions by Wynn, Suchman, Gerson, and Star provided the 'exemplars', in a Kuhnian sense, for defining a new research program in which in-depth studies of cooperative work 'in the wild' were considered a prerequisite for developing computer technologies for human interaction. However, we should remember that new research paradigms are not necessarily heralded as such when they arrive on the scene. In fact, as pointed out by Kuhn, 'we must recognize how very limited in both scope and precision a paradigm can be at the time of its first appearance'. Thus the 'success of a paradigm […] is at the start largely a promise of success discoverable in selected and still incomplete examples.' (Kuhn, 1962, pp. 23 f.). This observation certainly applies to the emergence of the practice-oriented research program of CSCW.

The exemplary role of these studies were not only a function of the findings or of the role of field work in producing them. In both cases the research was integral to settings in which computer scientists and sociologists were addressing the same set of problems. The work of Suchman and Wynn was, of course, an important part of the research at Xerox PARC (from where the computer workstation and the Ethernet originated) where Suchman would later head a highly influential interdisciplinary group of researchers. It is less well known but important to note that the work of Gerson and Star anticipated much of was later to unfold in CSCW in that their research was part of a collaborative research network involving both sociologists and computer scientists. The network, which *inter alia* also included Carl Hewitt, Anselm Strauss, Rob Kling, and Les Gasser, brought together sociologists with a track record in workplace studies of health care and biological research work *as well as* computer scientists engaged in developing what would later be known as distributed AI and agent-based architectures.

So, when Liam Bannon and I wrote our programmatic article for the first European CSCW conference in 1989, *this* was the kind of work we had in mind: 'CSCW should be conceived as an endeavor to understand the nature and characteristics of cooperative work with the objective of designing adequate computer-based technologies. [...] The focus is to *understand,* so as to *better support,* cooperative work.' (Bannon and Schmidt, 1989, p. 360).

In sum, two intellectual movements merged in the formation of CSCW. On one hand, CMC (as a technologically oriented research program) had arrived at a stage where it was beginning to dawn on many participants that the program was barking up the wrong tree. It had been focusing on aspects of interaction ('communication') that were conceived of as divorced from work practices but which normally are an integral part of doing the work and deeply enmeshed in the materiality of the setting and its organizational conventions and procedures. To move beyond that impasse, it was found necessary to develop an understanding of actual cooperative work practices. On the other hand, the 'office automation' program had landed in a situation where it had become clear that formal organizational constructs such as prescribed procedures are not mere algorithmic subroutines but part and parcel of professional work practices. It was, again, found necessary to develop an understanding of actual cooperative work practices. Here the history of CSCW proper begins.

When I point to the early work of Suchman, Wynn, Gerson, and Star as 'exemplars' of practice-oriented contributions to technological research, this of course does not mean that the formation CSCW was not part of a wider intellectual movement than circumscribed by Ethnomethodology and Symbolic Interactionism. To the contrary. It was, and is, a distinct research effort within a much broader movement that, in different ways, strives to understand computing in its social context. Suffice it to mention the Participatory Design movement (e.g., Bjerknes*, et al*., 1987) that brought together computer scientists and others striving to understand the design and use of computing systems as embodied social practices. Likewise, subversive elements within Artificial Intelligence such as Terry Winograd quite early had serious doubts as to the conceptual foundations of AI and defected. At about the same time, a related movement away from cognitive science towards an 'ecological' and 'naturalistic' conception of computing was unfolding in Human Factors engineering. Consequently, when CSCW emerged as a distinct research program, it became a forum — and a rather tumultuous one at that — of these and other intellectual currents (Activity Theory, Distributed Cognition, etc.). When I nonetheless point to these early 'exemplars' it is because they, in different ways and from different intellectual traditions, demonstrated that in-depth studies of work practices could contribute not only to systems design but to the conceptual foundations of technological development.

1.1. CSCW's program

What was new in CSCW, then? Firstly, the idea of doing field work as part of 'requirements analysis' is not at all new. The design of the very first computer applications for commercial purposes (payroll systems, etc.) was based on studies of actual practices. As early as 1953, the requirements analysis for one of the very first business applications, the design of a program for the ordering of goods for Lyons Teashops in the UK, involved genuine field work (Ferry, 2003, pp. 121-129). What was new in CSCW has to do with the difference between the *development of technologies*, i.e., technical knowledge, methods, principles, etc., and *systems design*, i.e., the instantiation and configuration of a set of known technologies for a specific purpose. The novelty of CSCW was not the idea of doing requirements analysis as an integrated part of the process of building a particular system for a particular setting, incorporating an array of more or less well-known technologies, but doing workplace studies for the purpose of developing new technologies, that is, to make field work an integral part of the conceptual work that is essential to technological research. Hence it is also not reasonable to expect of each and every study of particular work practices that is concludes in 'implications for design'. What is required, however, is that studies should have *implications for CSCW*. The road from studies of work practices to technological development is indirect and complex. The role of ethnographic and other workplace studies in CSCW is not that of producing a requirements analysis but to contribute conceptually.

The fecundity of CSCW's practice-oriented program became evident immediately, even as the program was being tentatively articulated. The first report on the Lancaster group's study of air traffic control was presented to the incipient CSCW community in 1989 (Harper*, et al.*, 1989) and was quickly followed by the equally emblematic study of the London Underground control room (Heath and Luff, 1991). Nor did it not take long for it to become clear that these new insights would have radical implications for not only the development of certain classes of applications but for underlying computer technologies. This was, for example, made explicit with respect to the research area of distributed systems by Rodden and Blair in their classic paper from 1991. Referring to the 'the rich patterns of cooperation found in CSCW' depicted in the early harvest of ethnographic studies in CSCW, the authors summarized their programmatic argumentation by stating that 'existing approaches to control in distributed systems are inadequate' (Rodden and Blair, 1991, p. 49). The implications for technological research are profound:

> 'For example, consider the problem of shared access to resources. In most distributed systems this is dealt with by masking out the existence of other users. Hence sharing is transparent with each user unaware of the activity of others. This clearly contradicts the needs of CSCW. [...] The problem with this approach is that presumed control decisions are embedded into the system and hence cannot be avoided or tailored for specific classes of application. This is the root of the problem in supporting CSCW. Because of the dynamic requirements of CSCW applications, it is very unlikely that such prescribed solutions will be suitable.' (Rodden and Blair, 1991, p. 59)

Rodden and Blair concluded that 'CSCW demands a fresh approach to control which is specifically tailored for cooperative working' (Rodden and Blair, 1991, p. 60). This was a crucial programmatic proposition. The key problem for CSCW is not 'communication' or 'resource sharing' but the cooperating actors' control of their interaction and, by implication, of the computational regulation of their interaction. This problem is fundamentally different from the issue of user control of system behavior in HCI, in that control in cooperative work settings is, in principle, distributed. This problem has since then been spelled out and elaborated under from different perspectives: 'event propagation mechanisms' for 'awareness' support, 'coordinative artifacts and protocols', and so on.

As observed above, the paradigm of the research program that is CSCW was exemplified by the early studies by Wynn, Suchman, Gerson, and Star which demonstrated how sociological inquiries could address conceptual issues in technological research. Similarly, with Rodden and Blair's re-conceptualization of fundamental issues in distributed computing CSCW's research program had been complemented by an exemplar of the correlative technological research. The reciprocality of the contributions of sociology and computer science respectively had also been exemplified.

1.2. The afterlife of CMC in CSCW

The developments within CMC research after the crisis and the formation of the CSCW research program are complex.

In a sense, the pattern of original CMC technology development has repeated itself, again and again. The case in point is of course the development of the Web (HTTP and HTML). It was initially developed by scientists at CERN for their own use, and the initial motive was almost identical with that of the Internet: access to resources across platforms. The technologies were themselves derived from previous technologies such as hypertext and markup languages. However, as with network email, when it arrived the Web was soon adopted by others to be used in other contexts (Gillies and Cailliau, 2000).

The pattern is characterized by occasional technological innovation, innovative applications of well-known technologies, often in novel configurations, and a significant element of reimplementation for other purposes in new contexts. As a result, wave upon wave of seemingly new communication facilities have, again and again, caught the attention of the media and the public at large: instant messaging, text messaging, chat, blogs, and so on. Some of them, such as instant messaging go back to the early days of time-sharing operating systems. What is new, however, is that they have been somewhat standardized so that they can be used across different platforms and, consequently, have been adopted by a mass audience. Similarly, in the case of chat and blogs, we are talking about facilities that are reimplementations of computer-conferencing and 'bulletin boards' *anno* 1980. What has given the scaled-down computer-conferencing idea a new lease on life is the ubiquity of the web browser: the HTTP protocol has become a general platform-independent way of establishing conversational sessions that are then governed by other communication protocols. And again, as a result of the 'superplatform' provided by the web browser, these conferencing facilities have been picked up on a mass scale too. That is, what is generally happening is that well-known computer-based communication technologies, often in innovative configurations, are reaching a mass audience.

The relationship of CMC research to these developments is not less complicated. But typically the new implementations have been undertaken by designers for their own use in their own particular part of the woods, only to be picked up and spread in a classical innovation-diffusion pattern. In a way that is reminiscent of the pattern of the '70s and early '80s, CMC research, in its many forms, strives to investigate possible 'effects' and 'impacts' of these socio-technical phenomena, but without the original's relatively close coupling to experimental technological development. This is not surprising, since the socio-technical phenomena under investigation typically do not represent new technologies but rather new applications on a mass scale.

It is here important to point out that, over the last ten or fifteen years, a large and diverse area of research, normally *also* referred to as CMC research, has emerged that does not have any relationship with technological research and does not consider itself related to the concerns and issues of CSCW. To use the wording of the program statement of one of the leading journals in this field, this research 'is concerned with the empirical study of human behavior in the online environment, and with the impact of evolving communication and information technology upon individuals, groups, organizations, and society'.[1] — The reason why this research area refers to itself as CMC research is simply that CMC technologies underpin the media that facilitate the behavior under investigation, just like other fields of communication and media studies investigate behavior connected to movies or TV. Such inquiries may be worthwhile, although inquiries that aim at understanding or even anticipating the societal impact of technical innovations are in a methodological muddle: 'much of the CMC work still holds to an overly determinist view concerning the role of technology in human affairs, attempting to assess impacts of new technology in general, and missing out on the interplay of social forces in the acceptance and use of the new media' (Bannon, 1992). Anyway, this body of CMC research addresses problems and conceives of its findings in ways that have no direct bearing on CSCW, and it is not my concern here.

My concern here is with the fact that CMC research continues within CSCW unaware of and unaffected by the fact that its conceptual legitimacy has been fundamentally challenged. Although the CMC paradigm's focus on communication as a separate activity has been found wanting and its model 'broken', and although this realization, together with the experience of the OA program, has given rise to a new research program with an entirely different paradigm, CMC research has continued in CSCW unabated, as if nothing has happened. In fact, it is becoming predominant, at least in quantitative terms.

Now, in so far as CMC facilities are adopted in work settings, which they obviously are, they may of course be of some interest to CSCW, as facilities we can build on or otherwise have to relate to — on par with database systems, network facilities, modelling techniques, or sensor and actuator technologies. Furthermore, the appropriation and use of CMC facilities and techniques in work settings may raise many interesting issues. These facilities are, for example, being deployed in ways that may change organizational boundaries and roles, blur the traditional separation of work and leisure in terms of time and place, and so on. These are issues that occupy researchers in economics, occupational sociology, and organization theory but they do have implications for CSCW in as much as they affect the organizational and material settings of cooperative work.

It is, in this context, also of relevance that net-based communication facilities are being employed to enable increasing geographical distribution of work in the form of, e.g., global production networks. These developments raise questions concerning the organization and management of cooperative work in such dispersed settings (cf. Hinds and Kiesler, 2002). These are important issues. Indeed, coordinating interdependent activities across space is one of the problems faced by actors engaged in cooperative work 'in the wild'. However, the model of com-

1 *The Journal of Online Behavior*: 'Overview' (http://www.behavior.net/JOB/job.html). According to the journal's editors, topics typically investigated in this area are: 'The role of the Internet in national and local news media use', 'The relationship between exposure to Internet pornography and sexual attitudes toward women', and 'Reformulating the Internet paradox: Social cognitive explanations of Internet use and depression'.

puter-mediated communication 'breaks down' (again) when the issue is investigated in abstraction from the actual coordinative practices of, say, software engineering work. And at any rate, handling cooperative interaction across geographical distance is only one issue in the coordination of interdependent activities, and it is thus absurd to define CSCW in terms of the issue of distance.

That is, for studies of CMC facilities to contribute to the technological commitments of CSCW, they would have to investigate how these facilities are appropriated in actual coordinative practices, that is, how practitioners integrate these facilities with their repertoire of coordinative artifacts, in their embodied activities, in material work settings. However, this kind of investigation falls well outside of what could be called 'the divorced-communication paradigm' that characterizes CMC research in CSCW.

What characterized CMC research in CSCW is, first of all, that it conceives of communication in abstraction from actual cooperative work practices. This is a fundamental precept inherited from the original CMC research. It defines its 'world view': what is considered relevant and perhaps even researchable. Secondly, CMC research focuses on computer networks as a means that facilitates interaction with only rudimentary computational regulation, as facilities on par with television and radio or telegraph and telephone (Olson and Olson, 2003, p. 584). In that sense, the program is faithful to the received 'medium' metaphor. But at that level of abstraction, e.g., in terms of 'media characteristics', no contribution to the development of technology is possible. Thirdly, however, in contrast to original CMC research, CMC in CSCW is reactive, conceiving of empirical work as something *post hoc* to technological development (as a kind of technology assessment).

As described above, CMC research formed in the '70s to investigate the new communication technologies that were being developed by computer technicians for their own use (in building, operating, maintaining whatever software and hardware systems they were working on) or in some cases deliberately designed for the use by others. CMC research anyway formed in close coupling with these development activities, sometimes carried out by the technicians or at least in close collaboration with the technicians. However, as email and the other forms of CMC technologies became standardized services, CMC research was left dangling. But when new applications of CMC technologies began to occur, especially spurred by the emergence of the Web, the methods and techniques of the original CMC research program were found applicable again, only now the continuation of CMC evaluation work had lost its connection to design and became a special blend of technology assessment and technology transfer. As pointed out by Bannon in 1992, 'the orientation of much of the CMC work is on evaluation rather than on gathering material to be used for design or re-design of technologies. Research is thus more reactive than pro-active. This affects the kinds of research methods used, the problems addressed in research, etc.' (Bannon, 1992).

What has remained constant in CMC research is the 'the divorced-communication paradigm'. Thus, for the purposes of CSCW, CMC research is marginal at best, a distraction at worst. In sum, CSCW as a community comprises not only different research programs but incommensurate paradigms.

What, then, drives the CMC research program in CSCW? An obvious reason is of course the wave after wave of seemingly new CMC facilities that fascinate the public and researchers alike. Another, perhaps supplementary, explanation of the unabated continuation of CMC evaluation studies in CSCW would be that many researchers have retained strong disciplinary reservations towards ethnography and other forms of workplace studies. For example, some CSCW researchers claim that the central role that ethnographic studies of actual practices holds in CSCW is in fact a source of 'weakness' of CSCW, and they advocate a 'stronger orientation' to what is claimed to be 'a large body of well-validated principles about human behavior in group and organizational contexts' that, correspondingly, employs 'data collection and analysis methods that emphasize parsimony and identification of generalizeable features of human behavior'. The aim of this, they state, is to develop 'universal principles of CSCW design' (Finholt and Teasley, 1998, p. 40 f.).

A discussion of the assumptions underlying this criticism of the role of ethnography in CSCW is of course beyond the scope of this paper. But a couple of points need to be made. First of all, it would of course be absurd to claim that just because a particular study of a cooperative work settings is based on 'ethnography', for instance by virtue of somebody's having been at the site for some time and observed events, then it is a valid contribution to CSCW, whereas a study that employs quantitative techniques is ruled out. Whatever the actual investigative technique, the issue is rather the specific analytical stance of the study: Does it provide an in-depth analysis of the *logic of the work practice* in question? Having said that, I should point out that Finholt and Teasley seem to take for granted, without reflection or argument, that there is one and only one legitimate form of scientific generalization, namely that of identifying abstract universal principles (e.g., 'laws'). Such an assumption is not only evidently false, as it would outlaw scientific insights of great value in a range of research fields. But in our context such dogmatism would lead to impotence. Indeed, in the context of cooperative work practices, such abstractions would be meaningless. Let me be a little more specific. Their criticism of the dominant role of ethnography in CSCW begs the question how one, on the basis of 'universal principles of CSCW design', can devise technologies that regulate *historically specific* professional work practices or take account of what Rodden and Blair called the 'rich pattern of cooperation'. Could general 'principles about human behavior in group and organizational contexts' tell us *anything relevant* about the contingent handling of production planning systems in manufacturing, the development of naming schemes in engineering, the coding practices of medical records, the role of flight progress strips in air traffic control? But such questions are of course inconceivable within the CMC world view.

1.3. Implications for design…

George Bernard Shaw is often cited for having remarked that 'Britain and America are two nations divided by a common language'. The same is true of CSCW. But here the confusion has implications of greater import than the occasional misunderstanding between Americans and Brits.

The fragmentation of CSCW is harmful. While mutual indifference between different schools may be acceptable in a field that does not aim at contributing to the development of technology, it is fatal to a field like CSCW. It fosters confusion and discontinuity; it makes it exceedingly difficult for the field to work in a cumulative or converging manner. The effect of that, in turn, is that CSCW is seriously handicapped in meeting its commitment to the development of computer-based technologies by means of which members of ordinary work settings can control the computational coordination of their distributed and yet interdependent activities.

Acknowledgements

The research has been supported by the Danish Research Council for Technology and Production (CosmoBiz, Grant no. 274-06-0415) and the Danish Strategic Research Council (CITH, Grant No. 2106-07-0017). I thank the six anonymous reviewers as well as the two 'shepherds' appointed by the Program Committee for criticisms and comments that helped me to make the thrust of the article come across more clearly. Last, but not least, I would like to express my gratitude to Liam Bannon for trying to instill in me a sense of the finer nuances of CMC research.

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